

January 17, 1958

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Dear Boris:

To yours of the 3rd.

Fallen back in the routine? Yes. Perhaps "enmired" would be the better term.

I realize you have some difficulty in reacting to Bernie's criticism. It is of course up to you to decide. Perhaps you should mention in the introduction that the work was pre-permease, and you should devote more time in the discussion to the permease hypothesis. I disagree with Bernie that you should not set up "straw men" to knock down - that is precisely how one does evolve a correct hypothesis. Perhaps he lacks some historical perspective.

I can understand the proposal of making two papers. But one should be preferred if possible. In the end it may appear that your most valuable contribution will be an assessment of the permease hypothesis. But in further perspective, it is not the hypothesis but the phenomenon of activation that has to be decided on. I am rather unhappy about a too glib view of the permease story, and I would stress, for example, the failure of azide (which poisons permease) to modify the kinetics with intact cells!

If you are continuing with permease studies, I hope you find a way to attack it at its fundamentals. Is the permease an actual pump, which facilitates the initial entry of the substrate, or is it merely a mechanism for concentration by inhibiting diffusion out? I do not think Monod has faced these fundamental questions satisfactorily.

Another anomaly: Lac^- is supposed to be merely a permease-deficient mutant. But there is supposed to be the same permease for galactose. Still Lac^- ferments galactose as well as Lac^+ . However, this point should be studied quantitatively! If it would be any help we could also send you $Lac^+ Gal^-$ (which lacks galactokinase) and also $Lac^- Gal^-$.

I think you would do well to revisit the States. But August is a terrible time. We will probably be at the International Congress of Microbiology at Stockholm then. I'll be sorry if we should miss you.

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I hope that circumstances never beat you down to be less than a perfectionist! Your friends admire you for it, and you are better appreciated (for good and for bad) than perhaps you know.

We have no thymine-dependent K-12; every now and then we look for it.

The lab is a little smaller now - Tetsuo Iino is still here, and will finish his excellent Ph.D. thesis very soon. Bob Wright has gone back to Australia, and has a more or less satisfactory job at Melbourne. But first he will spend 6 months with Ephrussi. Alan Richter is working on the specificity of some Hfr's. The post-doctoral fellows last year - the Orskovs from Denmark and Heumann from Germany have left. There are also two girls in the lab, Jackie St. Clair and Ann Cook. We are hoping that Cavalli will be here a few months this spring.

"Medical Genetics" is still moving along, its staff is only Morton and me so far. We are having a symposium (not at my initiative, though it's probably not a bad idea) in April. Plans are completed for another research wing at the hospital. Until that is completed, my own lab stays here. For myself, I am still following up the penicillin work, have so far been unsuccessful in transduction with DNA (contra Chargaff in Nature, Oct. 26, '57) which was a sloppy job, and not at all convincing. You will get some reprints and a ms. very soon.

We are anxious to keep in touch with you. All best wishes to yourself and Rafael.

Yours sincerely,

JL/ew